DISCUSSION ON

"COMPARISON OF IONOSPHERIC RADIO TRANSMISSION FORECASTS WITH PRACTICAL RESULTS"*

BEFORE THE RADIO SECTION, 16TH JANUARY, 1952

Mr. J. A. Smale: I have been interested for some time in what I call the "post mortem" side of propagation and prediction work, and I have been anxious to see some real use made of the mass of evidence which we have collected in our organization over a number of years. We are fortunately placed in having stations all over the world at which we are able to make observations.

The practical aspect of the paper disappoints me. Very valuable work has been done in attempting to diagnose failures in the method of prediction from the data available, but only one part of the necessary prediction—the m.u.f.—is dealt with; the much more difficult problem of the l.u.f. is dismissed on the score that insufficient evidence was available on which the authors could base any conclusion. It is no doubt the more difficult part, but it is more attractive to the engineer to have information on which he can estimate the power required than the frequency on which it is required. There is more money involved in differences of power than in differences of frequency. I hope that it will soon be possible for all the data available in connection with l.u.f. to be analysed by the authors. They would in consequence make an even greater contribution to the solution of the practical engineer's problem than they have done in the present paper.

By operating always near the m.u.f., the effect of attenuation and noise are avoided to the maximum extent, but in practice this means many interruptions of service for frequency changes on the equipment and many frequency allocations for each service—a quite impracticable situation. The work of the Provisional Frequency Board (Geneva), set up following a recommendation of the Atlantic City Conference (1947) of the International Telecommunications Union, failed largely because circuits were over-engineered for many frequencies, whereas years of experience showed that two frequencies sufficed in many cases for adequate service.

The standard deviation of about 18–20% given in the paper was confirmed by some 300 odd circuit-months' analysis; i.e. 12 circuits for the three years, 1947, 1948 and 1949. The standard deviation from the whole of these results was judged to be some 15%, which is a little lower than the figure arrived at by the authors.

The circuit data on which the authors have based their analysis were taken in years of high sunspot activity. This may have obscured certain other and perhaps more serious errors due to the high m.u.f.'s prevailing at that time. It may well be that further analysis of the circuit data for 1950–51—years of decreasing sunspot numbers—may throw some more light on the matter. In the last two summers it has been obvious that the actual day-time values of m.u.f. for eastern circuits have been far from agreement with the predictions. Much higher frequencies have been employed, resulting in errors of something over 20%, a 3- or 4-Mc/s difference in m.u.f. That is important to the practical engineer, because these predictions are issued some months beforehand, and although corrections may be made and issued at the last minute, large changes in the frequency requirements of the circuits lead to inconvenience and to requests for new frequency allocations, which are difficult to obtain.

Predictions are of great value to the engineers responsible for operating services, but they would be of much greater practical value if the post-mortem data available were actually used to refine the fundamental data on which the predictions are based.

Mr. K. W. Tremellen: In Section 2.1, I think that the statements "enable the diurnal and annual changes . . . to be quite well understood" and "has allowed the relation between solar activity and the regular regions to be worked out" may be a little optimistic.

In Section 2.2, the statement "the two-control-point method gives results of greater accuracy" is satisfying to me, since I was the instigator of this method, but please do not ask me to explain it.

In the same Section, some lines later is stated: "some allowance would have to be made for off path transmission, scattering and reflections from Es-region." I think this is going to be very difficult, if not impossible.

With regard to the day-to-day fluctuations in critical frequency, 13% is given as the figure obtained at Slough, and I think the figure of 15% which has been universally accepted for many years is probably near enough for world-wide stations.

One point which I think may be missed is that some of the parameters used in constructing the charts may be completely wrong; nevertheless, when combined, these may give a correct forecast. Furthermore, prediction charts produced by two different people may give the same predicted value of m.u.f. for a circuit, although the control points at either end of the circuit may have different values on the two charts.

I have found a short-wave Adcock direction-finder of great use in detecting the presence of off-path or scattered signals, and some 20 years ago I made an analysis of the London–Vienna circuit which showed that the latter operated on scattered signals in some cases for two hours after the direct-ray signals on the true great-circle path had faded out.

Finally, since in place of a placid reflecting layer we appear to be accepting the view of the ionosphere as a complex and irregular structure subject to tides and drifting at times at 300 m.p.h., I think the accuracy of the predictions is truly remarkable.

Mr. T. W. Bennington: The authors have made a detailed examination of the errors which are introduced, stage-by-stage, into the m.u.f. forecasts during the course of their production. I should like to suggest, however, that one of the errors for which the standard deviation is given as "unknown" in Table 1 can very often constitute the major difference between predictions and observed results, and can at times assume such proportions as to dwarf all the other errors listed in that Table. I refer to "abnormal" propagation, i.e. a mode of propagation whose effects cannot be accounted for by the normal F-layer mode.

In the summer day, the vertical-incidence critical frequency for the F2-layer decreases as compared with that for the winter day, and this leads to a large decrease in the predicted m.u.f. for long-distance propagation during the summer months. In the B.B.C.

* Paper by A. F. WILKINS and C. M. MINNIS (see 1951, 98, Part III, p. 209).

we have found that, at least over circuits running in southerly and easterly directions from Britain, the higher of the frequencies which we use during the daytime do not deteriorate in the summer when they exceed the predicted m.u.f. The result is that we work for many hours during several months of the year on frequencies which are as much as two broadcast bands above the m.u.f. predicted for any time of the day. We continue to use these frequencies during the summer months because we have continuous reports from skilled observers that they are consistently well received, and there is therefore no point in using lower frequencies, even though the predictions indicate that this should be done.

For example, during the period May–August of both 1950 and 1951 we transmitted to Malaya for a mean time of 20·5 h per day, i.e. total transmission time on all frequencies. Most of this time was during the daylight hours. A post-mortem analysis shows that our frequencies were entirely above the predicted m.u.f. for 44·7%, and above the predicted o.w.f. for 71·6%, of this time. Yet, according to the log sheets received from Singapore, reception of these frequencies was unsatisfactory for an extremely small proportion of the total time.

This is, of course, the worst side of the picture, for during the equinoctial and winter months the transmissions normally took place on frequencies below the predicted m.u.f. I should add, however, that we do not know for certain whether or not the same effect would occur if we worked above the predicted m.u.f. during these months. We have not noticed it because, since the predicted m.u.f. is higher, we have little occasion to do so.

I think that by using their method of prediction, the authors will miss the discrepancies which we in the B.B.C. have noticed. They emphasize that the difference in the time between the predicted and actual fade-in or fade-out can be a very misleading index of the accuracy or consistency of ionospheric forecasts. However, I observe from the RSGB figures given in Table 3 that there are large discrepancies between observed and predicted median values of m.u.f. for southerly and easterly circuits, which tend to confirm what we have noticed.

The authors say that their comparisons lead them to conclude that "there is, in general, no wide divergence between predictions and actual ionospheric conditions as judged by commercial experience." As I have indicated, our experience in the B.B.C. would hardly lead us to agree unreservedly with that remark.

Mr. C. F. Sutton: I should like to ask the authors whether very-short-term amending factors based on current sunspot activity could not be signalled to terminals to reduce misuse of frequencies, and whether they have any plans for using long-distance transmissions to supplement vertical-incidence observations. There must be idle transmitters whose services could be useful; the WWV and RSGB results show what can be done.

To check ionospheric forecasting, the Services are beginning to reap the benefit of centralized traffic analysis. We are finding that operation by three independent authorities of somewhat similar links gives useful clues from traffic details to propagation peculiarities, and to operational and apparatus troubles which are not likely to occur simultaneously.

I should like to confirm what Mr. Bennington has said about the Singapore circuit; throughout the same period the Ceylon circuit operated in much the same way.

From the user's point of view, it would seem desirable to focus attention on major discrepancies, as distinct from taking too much from correlations of fade-in and fade-out times. In the latter, day-to-day variations about the mean are unpredictable and must be dealt with by *ad hoc* means, such as the pilot transmitter, whereas sustained high-running involves considering a substantially unvarying factor.

In this connection, however, I would point out that the RSGB figures listed in Table 3 may include skip or near skip cases, probably non-great-circle signals. Teleprinters could not possibly look at skip signals without considerable distortion.

Mr. J. K. S. Jowett: In Section 2.6 the authors suggest that the commercial user is less interested in the error in frequency prediction than in the consequent error in the time of fade-in or fade-out on a particular frequency. I should like to question that. Certainly the user is concerned with the commercial time lost due to errors in prediction, but, on a point-to-point service, the lost time due to an unexpected necessity to change frequency may be no different whether the predicted fade-out time is one hour late or four hours late. Even for broadcast services, for which frequency schedules are fixed in advance, I suggest the authors' statement requires qualification, for here the service is presumably operating always below the o.w.f. and the time actually lost may not be as much as A/B hours but rather only $(A - 15)/B$ hours.

The apparent success of the two-point-control method in providing predictions of circuit m.u.f. is commented upon in the paper. As is well known, this method does not rest upon any assumptions as to the mode of propagation between the two control points, 2 000 km from each terminal. I suggest some experimental work involving simultaneous measurements of wave-arrival angle and field strength over a multi-hop path would be valuable in throwing some light upon the causes of its general success and occasional failure.

We in the Post Office have found, on the transatlantic path, that the fade-out due to penetration on frequencies around 15 or 20 Mc/s often occurs very gradually. The received field-strength may fall steadily over a period of up to three or four hours before the circuit becomes unusable, and at least three reasons may be advanced for this. It may be due to increasing deviative absorption as the m.u.f. is approached, or to the progressive elimination of multiple propagation modes, or, finally, it may result from the inadequate low-angle radiation properties of the aerials used. I suspect that often the first factor does not play any great part in the process; however that may be, it is of great practical importance to know to what extent the last of these factors is responsible for the gradual decrease of signal. I should be glad to have the authors' comments on this question and to learn their views on the possibilities of investigating the matter further by field tests. I should perhaps add that we have frequently observed, at our musa receiving station at Cooling, wave-arrival angles considerably lower than those corresponding to simple multi-hop propagation conditions from the F-layer.

On the question of investigating further the accuracy of m.u.f. forecasts, I suggest that commercial circuits are unlikely to yield much more information of real value in confirming the predictions. The inaccuracies resulting from studies of commercial data are, I consider, often of the same order as the inaccuracies of the forecast themselves. It would therefore seem far better to concentrate on the reception in Britain, and in other parts of the world, of standard frequency transmissions such as WWV. Thus we should have a controlled experiment for which time of fade-in and fade-out could be accurately recorded and analysed.

Mr. K. S. W. Maunder: I must confess to feeling some dismay on reading that the standard deviation of fade-out time discrepancies was estimated to be as much as 4 hours. Unless this statement is given some perspective, it is likely unnecessarily to diminish our faith in predictions. I realize, of course, that the authors have sounded a note of caution.

It seems that this statement rests on Table 1 and the m.u.f./time gradients, but the unknowns and the rough assessments concerned in these do not warrant confidence in the overall estimate. Incidentally, with regard to item (ii) in Table 1, I feel strongly that, since the predictions profess to represent monthly values

(median), any error should be assessed as a difference between monthly values, predicted and observed, so that the correct allowance for the standard deviation in the case of the day-to-day fluctuation should be nearer 2% than 13%.

The authors state in Section 3.3 that examination of the data presented results in values of time differences consistent with the estimate obtained from Table 1. My own impression based on a limited analysis of Service circuits is that it is somewhat smaller, being between 2 and 3 hours, or perhaps about one-half of the period given by the authors.

Even so, I am not satisfied that the usual practice of combining data from various circuits, whereby the overall mean time error becomes near zero, really produces a significant standard deviation. I should like to ask the authors whether they have investigated this point, because I have found that the mean time differences for individual circuits are usually not zero and are often of the same order as the standard deviations about the means.

The mean time difference in such a case implies a systematic error, either in the prediction or the operator's working of the circuit. The r.m.s. value in the case of the lumped data can then be a misleading quantity, because it is not truly representative of either systematic or random errors, although depending on both.

I should like to present data which illustrate this point. The data are derived from circuit frequency records by a median time analysis (instead of one of average times) because the median time of use of a frequency is validly comparable with a graph representing the median frequency, whereas the average time is not. Analysis of this type has the advantage that a median time can often be obtained when the operating frequency is not in use on all days of the month, and also that information on more than one frequency is usually obtained. The analysis regards the times of change from one operating frequency to the one adjacent, either up or down, as the determining events in the daily operation of the circuit. Analysis in respect of one circuit for one

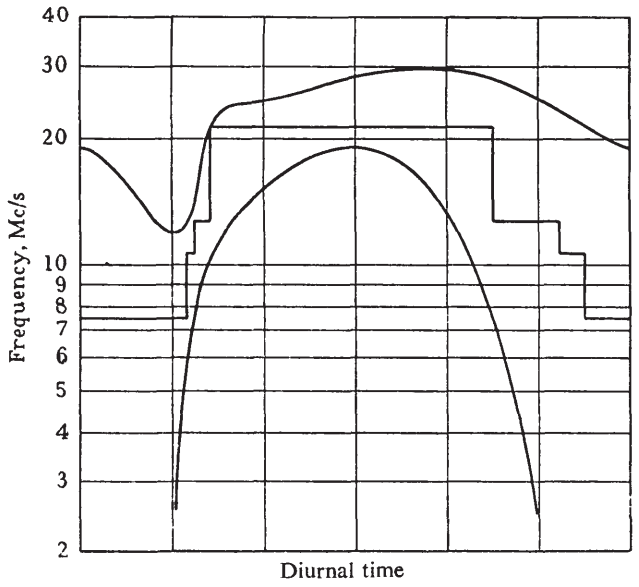

Fig. A.—The relationship between the median monthly frequency usage and the maximum and lowest usable frequencies.
Simonstown-United Kingdom circuit, July, 1948.

month yields a step-graph of the type illustrated in Fig. A, which is then comparable with the median predictions of both m.u.f. and l.u.f., as shown.

The mean time differences and standard deviations arising from the data of five circuits for 12 months are shown in Table A. It

Table A

MEAN AND STANDARD DEVIATION OF TIME DIFFERENCES

Circuit between the United Kingdom and	Fade-in			Fade-out		
	Mean*	Standard deviation	No. of circuit-months	Mean*	Standard deviation	No. of circuit-months
	h	h		h	h	
Ceylon	− 0·93	1·58	33	− 0·01	3·35	33
Halifax	− 1·74	1·53	19	+ 1·46	1·40	19
Simonstown ..	− 0·49	1·02	35	+ 1·77	2·90	35
Gibraltar	+ 0·44	1·04	17	− 1·91	2·22	17
Malta	− 1·86	2·59	9	+ 0·73	1·31	10
Mean and standard deviation for all frequencies and circuits	− 0·80	1·63	113	+ 0·56	2·84	114

When the predicted median time is later than the observed median time the sign of the time difference is positive, and vice versa.

will be seen that there is a clear tendency to systematic error, which, as I have said before, cannot be attributed entirely to the predictions. For instance, the dominantly negative sign of fade-in errors is almost certainly due to user error, and one cannot be sure that the positive sign for the fade-out errors is not due to the same cause unless one has exact knowledge (in this case lacking) of the circuit procedure and discipline.

I agree with Mr. Smale that the Service or commercial circuit is a poor instrument for testing the accuracy of predictions and feel that we must find a better method of doing so than that of comparing predicted and observed fade-in and fade-out times.

Dr. R. L. Smith-Rose: There is a difficulty in which any organization responsible for running a forecasting service of this sort continuously finds itself, in that it is not easy to get information about the successful forecasts, but attention is always drawn to the cases that have gone wrong. In our case, in particular, forecasting work is very difficult, because of the sparseness of the information available. If you look at a map of the world on which all the ionospheric observatories are marked—and there are only some fifty of these—you will realize that there are vast areas of the world where we know nothing about the ionosphere. Combine with that the theory as to how waves get from the transmitting station to the receiving station, and the difficulties in which the radio transmission forecaster is working will be appreciated.

I assume—and this is a matter which I should like to put to users—that the forecasts are of some use to those operating radio circuits, even at the present somewhat limited accuracy. I wonder what kind of reception we should get if we were to decide not to issue any more predictions until we were satisfied that they were more accurate than at present. We are all very conscious of the limitations of the work, but it is only by plodding on in the way suggested in this discussion and securing co-operation from those who can help us that we can improve these predictions.

Sqn. Ldr. H. T. Regester: To illustrate the value that the R.A.F. attaches to the authors' work I should like to discuss the practical use of the optimum-frequency charts which in the R.A.F. we distribute to all main signals terminals at home and abroad, and upon which we rely for the times our frequencies should be changed. The technique is to change the standby transmitter to the next predicted frequency, or rather as near to it as our allocation of frequencies allows, an hour before the change is due. In the duplex method of working the two radio terminals depend on their opposite ends for instructions on when to change frequency, and these instructions are transmitted over the main

channel, assuming, of course, that communication is still satisfactory, which it often is not.

If the instructions fail to arrive before the main channel has faded out, it then becomes a matter of routine for both terminals to examine their frequency-prediction charts, decide on which frequency their opposite numbers will transmit next and then to make the necessary arrangements for receiving them.

This use of the charts demonstrates conclusively the reliance which the R.A.F. puts upon their accuracy. We know from experience that there are small discrepancies in the times of fade-in and fade-out and that, if anything, the m.u.f. is underestimated, but we have never had any real cause to doubt the accuracy of the frequency-prediction charts.

When the charts are sent out we recommend that the recipients use them as a guide only, not because we mistrust the predictions but because we feel that any average forecast for a month cannot hope to be accurate, since radio conditions vary so considerably from day to day. I proved this latter point by preparing graphs of the daily availability figures on our two most difficult circuits, those to Colombo and Ottawa, and I was astonished to find that the normal variation in the day-to-day figures was 3–4 hours for Colombo and up to 10 hours for Ottawa. We do not know the reason for this and assume that ionospheric disturbances cause the change in radio conditions, but the variations apparently form a pattern which at one time I thought followed the $4\frac{1}{2}$- or 9-day cycle suspected to exist in the Es-region. It will take many more graphs yet to prove whether or not a pattern is being followed, but when considering the large day-to-day known variations it is surprising that the predictions are as good as they are.

We have yet another use for the charts. We examine the prediction for two or three months ahead and check the frequencies which will be required with those already allotted to us by the Air Ministry, and if we find that a certain frequency is lacking, then we apply for one in the particular band. In fact, we have picked a number of frequencies in this manner.

Mr. E. L. E. Pawley: It is hardly necessary to stress that it is of the utmost importance in short-wave broadcasting that we should make the best possible use of the small amount of money and the small number of frequency channels which the Government vouchsafes to us. It would help us to do that if the errors in these predictions were put right. As they stand, the predictions provide a body of basic information which is most useful, but there remain these errors, and I believe it would be worth making a substantial effort to try to eliminate them by a study of the causes that have led to them.

It is true that one can obtain cabled reports from the receiving zones as a check that one is using the right frequency, but such reports give information only about the frequency being used and thus do not indicate how much better the effect would be if some other frequency were used.

A possible source of error which may be greater than the authors have indicated is that in the computation of the m.u.f. from the critical frequency at vertical incidence. There are a number of ways in which that error might be considerably greater than the figure of 3% quoted by the authors. For instance, in computing the m.u.f. one must calculate the geometry of the problem, and that necessitates a knowledge of the actual height at which the reflection occurs. The experimental evidence indicates the virtual height, and the difference between the one and the other is accounted for by the fact that the group velocity in the ionized region is lower than elsewhere. I wonder whether that correction, and hence the calculated angle of incidence, may not be seriously in error in certain cases by reason of the fact that the ionized layer is not a specular reflector but a region with a considerable depth and that one knows very little about the way in which the ionization varies through the layer.

There is perhaps another way in which the same calculation may be in error: where there are ionized regions below the F-layer, they may not only affect the velocity of propagation but also cause bending which will affect the actual angle of incidence at the point at which it occurs.

Mr. G. Millington: I should like to say a few words about the control-point method of determining the m.u.f. over a long-distance route where the maximum density in the layer varies considerably and may be much less in the middle than at either end. Although this is an empirical method, it seems to work in all but exceptional cases. How it does so is an unsolved problem. It appears that so long as there is sufficient density at the reflection points for the first and final hops, it does not matter if the density is too low in between. Even if it is well below the value required to reflect a ray at very oblique incidence, the signal gets through to the distant receiver. On the other hand, it is not receivable in the middle of the route, where the density above is low.

I think the explanation must lie in the fact that a ray treatment is inadequate in such a case and that it must be replaced by a wave analysis. As a suggestion, the mechanism may be that of a waveguide with an upper boundary that is a good reflector at the two ends but is somewhat leaky in between. As a result, the field-strength distribution with height is different in the middle section and most of the energy is at the top of the duct, where leakage will tend to occur. However, this leakage cannot be great, and the energy is guided round the earth until the good reflecting conditions are met again near the receiving end. Here, a redistribution of energy in the duct can take place, restoring the conditions at the transmitting end and bringing a large portion down to the earth again.

Captain C. F. Booth: With regard to the suggestion by Dr. Smith-Rose that it would be helpful if a number of fixed services, when not on traffic, sent pulses to assist the ionospheric worker to accumulate practical information, I regret that there are considerations which make this very difficult and almost impracticable. However, Mr. Jowett has referred to the possible use of standard-frequency transmissions to provide practical results, and this also is referred to in the paper. It is, in my opinion, a very important and useful suggestion. Unfortunately, at present there are only about three standard-frequency stations operating —one in Washington (WWV), one in the Pacific (WWVH) and one at Rugby (MSF), and, since they do not operate on a time-sharing basis and are transmitting signals of almost identical frequencies, it is not possible always to state the source or sources of the transmission received. Eventually it is hoped there will be sufficient stations to provide adequate standard-frequency services throughout the world, and that may necessitate at least six stations. If some form of time-sharing between the stations could be agreed upon, so that it would be known which station was on at a particular time, I am quite sure the ionospheric physicist would obtain extremely useful information regarding the transmission characteristics of particular circuits. If I may stress the point, this question should be borne in mind in any discussions that are going on in the appropriate study group of the C.C.I.R. It would also be desirable to arrange for a silence period of a few minutes' duration, every hour or two hours, in the transmissions, so that observations on the radio noise levels in the bands prescribed for standard-frequency transmission could be made without interference from radio transmissions; in fact these band allocations could be likened to windows through which radio noise could be observed without difficulty during the silence periods.

Mr. Pawley raised the question of broadcasting, for which it is more difficult to ensure that the optimum frequency is being used than is the case for the fixed service, and he stressed the point that it is important to make sure that we are making the

most effective use of our frequency allocations. The paper is very important in this respect. As many are aware, we have been considering the difficult question of the international allocation of frequencies at great length since the 1939–45 War. On the one hand there are many requirements for new services, but on the other there is a definite limit to the number of frequency allocations which can be made available for long-distance purposes. It is therefore very important for all users of the spectrum to make the maximum possible use of their frequency allocations, and anything the authors and their colleagues can do to ensure that we improve the efficiency of usage is vital. From that point of view alone it is important that the work described by the authors should continue.

Cmdr. Newell A. Atwood, U.S.N. (*communicated*): As the authors have pointed out, there are certain limitations upon the use of commercial and service data which prevent such data from giving a full and completely accurate account of ionospheric conditions. If a series of transmissions could be made at times and over periods selected in accordance with predictions in such a manner as to acquire data during the transition points from optimum conditions to unacceptable conditions, and conversely, such data would make possible a more accurate study of correlation between forecasts and results than can be obtained from commercial and service data.

During the latter months of 1947, I was associated with a project carried on by the United States Navy, during which it was possible, in a very limited manner, to compare predictions with results under conditions somewhat similar to those described above.

In order to evaluate the performance of several types of radio-teletype convertors, a series of 4-hour transmissions from San Francisco were scheduled for each day of 13 consecutive days in October, 1947, the frequencies of transmission being so varied that there were five made on 5 440 kc/s, two on 12 120 kc/s, three on 18 060 kc/s and three on 22 575 kc/s. Radiated power for these transmissions was 3 kW, approximately identical for each transmission, using a Navy transmitter with frequency-shift keying. These transmissions consisted of the automatic repetition of a single line of teletype using an endless precut tape to feed the keyer.

In Norfolk, Virginia, approximately 2 500 miles from San Francisco, signals were received on four identical Navy receivers connected to four different convertors—a standard Navy convertor and those undergoing evaluation. For the evaluation, the percentages of errors in printed copy obtained from identical teletype printers in combination with each of the convertors were compared. For a collateral study of predicted and actual propagation conditions, only the printed copy obtained from the teletype printer in combination with the standard Navy convertor was utilized, since this served as the control for the evaluation of the other convertors.

Previous tests having failed to provide data permitting comparisons between the quantity of acceptable results from each convertor (in part because conditions during the transmission periods used gave either a consistently strong or a consistently weak signal), the times selected for the tests in October for the scheduled 4-hour transmissions on each frequency were chosen such that the transmission would be made during a period when propagation predictions indicated that conditions would vary between optimum and unacceptable throughout the period.

The 18 060- and 22 575-kc/s transmissions were scheduled such that the operating frequency would first be below the recommended frequency, then between it and the upper limit frequency, and finally above the upper limit frequency. For the 5 440-kc/s transmission the times selected were chosen such that the operating frequency was initially higher than the lower limit frequency, and the schedule continued well past the time when the operating frequency was lower than the predicted lower limit. The 12 120-kc/s transmission was scheduled to be made at a time when the operating frequency always exceeded the lower limit frequency and never exceeded the recommended frequency.

The results obtained are represented graphically in Fig. B.

From the Figure it will be observed that in no case was readable copy received after the time at which the operating frequency exceeded the upper limit frequency, nor at the time when the operating frequency was lower than the predicted lower limit frequency. Additionally, it will be observed that, for the 22 575-kc/s transmission, the peak reception occurred at very nearly the time when the operating frequency equalled the upper limit frequency. For the 18 060-kc/s transmission, the peak reception occurred at the time when the operating frequency equalled the recommended frequency. Other correlation between predicted and actual propagation conditions will be apparent from the Figure.

It is believed that the results indicated represent a high degree of correlation between predicted and actual propagation conditions, during the rather brief period in which these transmissions were made. Similar studies over a much longer period and at various times during the sunspot cycle would, of course, be required before sufficient data could be obtained to permit the drawing of conclusions comparable to those presented by the authors. The study described above, however, indicates not only a desirable method of approaching this problem, but also a method for the use of propagation predictions in connection with equipment evaluation problems, possibly suggesting a new tool for a new group of users of ionospheric radio-transmission forecasts.

Mr. J. D. Holland (*communicated*): The authors' statement that no reliable information was available for most of the errors is remarkable in view of the agreement obtained between the predicted and actual times of fade. From results obtained on a recently established long-distance direct-printing circuit, I learnt that the forecasts could be relied on for about 25 days in each month. The information obtained from m.u.f. predictions, aided by careful interpretation by the station supervisor in estimating the correct moment to change frequency before printing errors become noticeable, has enabled this station to achieve a high availability time.

I think a good chance exists of reducing the magnitudes of the standard deviations by feeding more information back to the authors and other statisticians engaged on this work. The authors state in Section 3.2 that the data obtained from commercial circuits are unreliable. Surely much useful information, without disturbance of traffic or personnel, could be obtained by automatic monitoring of field strength, frequency and transmission power? There is very little hope of obtaining frequency allocations purely for continuous field-strength recording in an already overcrowded spectrum, and reliable commercial data would represent a useful contribution. Recently I made up circuits which attempted to record automatically the signal and noise levels during continuous transmissions. The general scheme is shown in Fig. C. The output from a receiver is fed into two filters, one, A, passing a very narrow band of frequencies centred on a bandwidth some 100 c/s wide, and the other, B, having maximum attenuation to frequencies passed by the first filter with zero attenuation to frequencies displaced $\pm$ 2 kc/s from mid-band. The spectrum is swept panoramically at a rate of about 30 c/s. The currents from both filters are combined in detectors in such a manner that the magnitude and direction of the d.c. output current flowing through the resistor R depends on the relative levels from the filters. This scheme is adaptable for continuous recording. During conditions of high noise level,

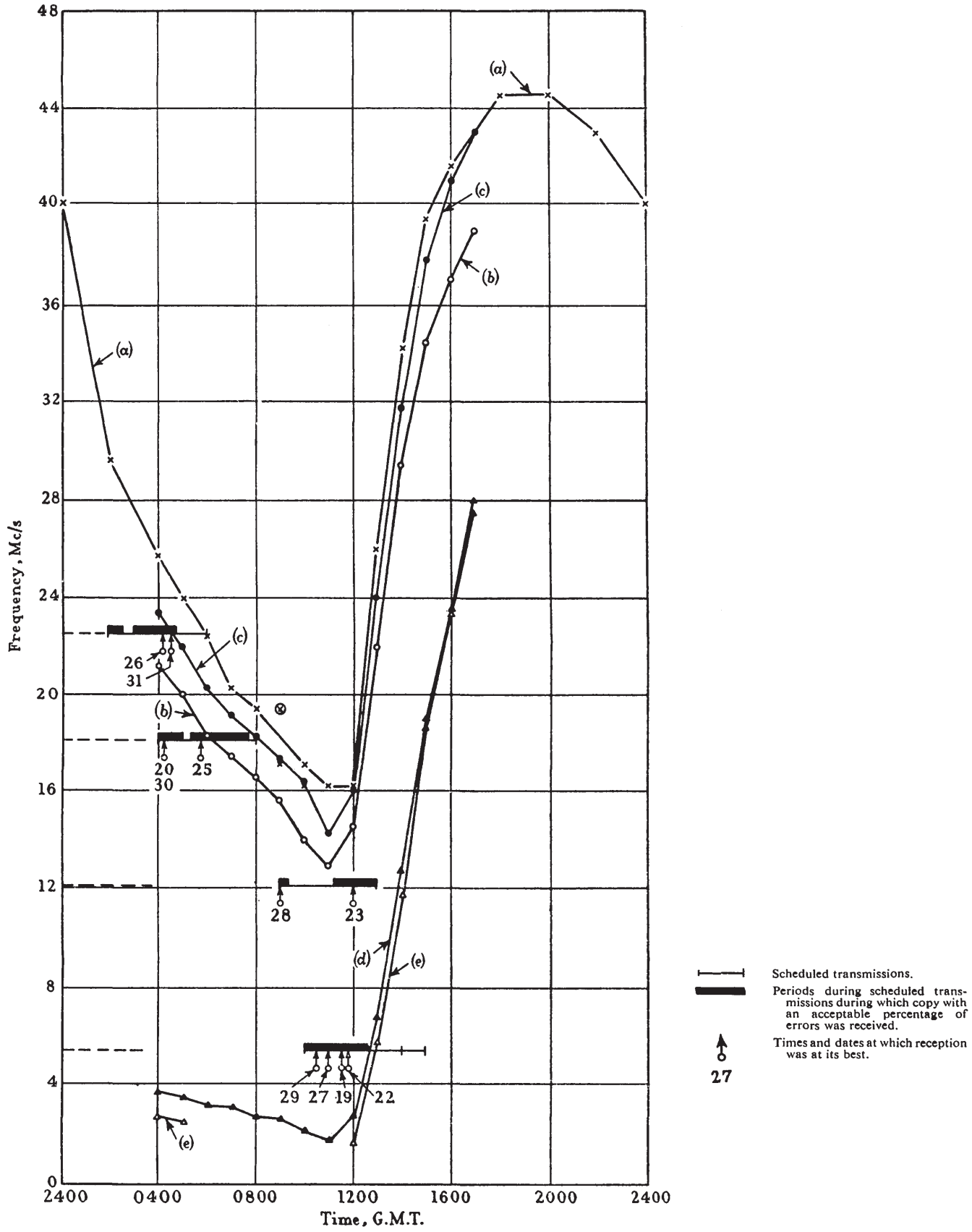

Fig. B.—Radio propagation predictions and observations over the San Francisco–Norfolk radio-teletype circuit, 16th–31st October, 1947.

(*a*) F2-layer m.u.f. computed from C.R.P.L. publication D35 with revisions for period 15th–30th October.
(*b*) Recommended frequencies from Navy publication NRPM-537 for October, 1947.
(*c*) Upper limit frequencies computed from a nomograph appearing in NRPM-537.
(*d*) Lower limit frequencies for 0·5-kW c.w. transmission, computed as in (*c*).
(*e*) Lower limit frequencies for 3-kW c.w. transmission computed from (*d*) using a radiated-power nomograph appearing in NRPM-537.

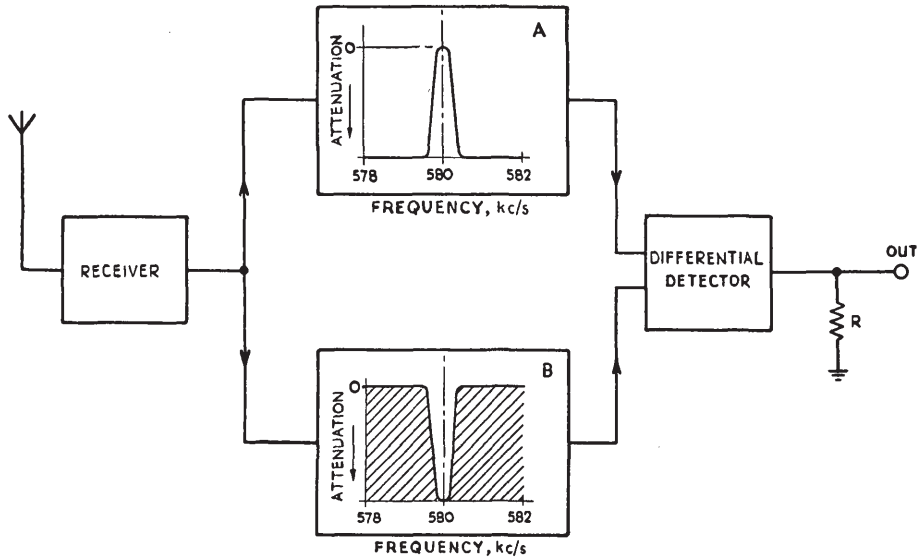

Fig. C.

in which the output consists of rectified noise components from filter B, a voltage is available for disabling the automatic frequency control system of the receiver.

I should like to know if the authors have any further information on the amount of frequency change due to Doppler effect on long-distance radio links operated in the h.f. band. The only reference* I have seen infers that the change in frequency is less than 1 c/s on a typical circuit. This question is related to the possibility of using very small frequency shifts, of 100 c/s or so, on telegraph circuits.